November 4, 1954

Dr. P. R. Edwards
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Dear Phil:

I am sorry if I have seemed uncommunicative about lw or anything else. The matter had just slipped my mind, and I was preoccupied with other matters. The main reason, perhaps, is that the results on lw seem so clear cut that there scarcely needed to be any further debate.

I don't underatand what Ole (Maaløe) is driving at; he has expressed his idea in a very murky way. At any rate, I can get nothing out of his suggestion that would be consistent with what we already know of the tramsduction homologies of the two phases and of lw stocks in particular.

Dr. Bernstein's experiments on lw were confined to those represented in the cultures we sent you some time ago, namely transductions to S. wien and S. dar-es-salaam. Since the b phase of S. wien was substituted in SW-1103 and SW-1105, these were not particularly informative at that, although the results were consistent with the formulation S. wien = H_1^{b} H_2^{lw}. SW-1104 was slightly more informative, as the replacement of lw of dar-es-salaam by a of sendai suggested that these were homologous, and d.-e.-s. = H_1^{lw} H_2^{en}.

Your own results are more instructive, particularly the transductions from lw:enx or lw:1,.. stocks to more typical diphasics, which resulted in the substitution of lw for its homolgue in the recipient. On this basis, fayed is clearly H_1^{lw}..., as is #408; while worthington is H_2^{lw}...

To be able to prove this in the most unequivocal fashion, it would be desirable to compare the transductions from one H_1^{lw}... stock and a second H_2^{lw} stock to a common recipient with characteristic H_1 and H_2 factors. If we had a suitable phage for #408 or dar-es-salaam and for wien, we could do this in group B, for example —x typhimurium. But the presently available data hardly admit of any other interpretation, provide one can rely on the regularity with which some lw donors will substitute this factor for a reliable phase-1 of the recipient, while others will substitute it for a reliable phase-2. This would mean, at the very least, to complete the picture that experiments similar to those in the table you sent me should be repeated several times. I do not think more extensive experiments are needed, but some of them (e.g. meleagridis —x bolton, and fayed—x newport) should be repeated intensively, often enough to be sure that there is a real, consistent differences in the substitution homology of the two lw phases.

I don't know what the context of your correspondence with Ole was, so this makes further comment rather difficult. Possibly his remarks are another way of formulating the idea of two loci, H_1 and H_2. He is right that one could not argue from the natural distribution of 2 lw as two whether there was more than one kind of determinant (the odd a:c diphasics point that up!). But the transduction experiments seem to show quite clearly that there are two kinds of lw phases, homologous with other phl and ph2 respectively, regardless of their (accidental?) serological identity. It would be rather as if the antigens h, n, x etc. were non-antigenic or not antigenically distinguishable in the rabbit, so that H_1^{eh} would be identified with H_2^{enx}. This suggests that it might in fact be worthwhile to try to analyse H_1^{lw} and H_2^{lw} in more detail, to determine whether there are some subtler serological differences. But there do not have to be any. The important thing for the success of this genetic theory is that transductions from a given donor give an unambiguous result as to the homologies of its lw phase. (repeated

If I hadn't before, let me thank you now for the cultures, phages, and serum. I don't think we'll jump in, just yet, into transductions involving other serological groups, at least until this IV XII— IV V XII business is cleared up. Right now, it is very much up in the air. Until we get some more meaty results here, I don't know how I could interpret your san diego changes.

I have not seen any Iseki reprints intended for you, as far as I know. I am sure he would send you a set on request. (His address is Dept. Legal Medicine, School ofvMedicine, Gunma University, Maebashi, Japan). I understand Lou Baron (in Landy's group at Wash.) has been working on this story too and has essentially confirmed it. (That is, on the 3,10—3,15 business). As far as I can tell, Iseki's transduction was with biochemical markers, and involved the same phage he used for the much more regular conversion of somatic antigens.

I wish we had an opportunity to talk all these things over personally; it is rather hard to get everything down on paper. Any chance of your coming out this way? If you could, I'd like you to see the Mating of Coli under the microscope, which is what has been keeping me (very) busy lately.

Yours as ever,

Joshua Lederberg

Lysogenic Strain	Donor	Recipient	Result
S. typhi murium SW 455	S. typhi murium LT-2' 4,5,12: i-1,2	S. dar-es-salaam #72 1,9,12: 1,w-e,n,z_{18}	1,9,12: i-e,n
S. typhi murium SW 456	S. typhi murium LT-2'	S. wien #281 4,12: b-1,w	4,12: i-1,w
S. newport 9-54	S. fayed #215 6,8: 1,w-1,2	S. marashino #55 6,8: a-e,n,x	6,8: 1,w-e,n,x
S. newport 9-54	S. fayed #215	S. newport 169-54 6,8: e,h-1,2	6,8: 1,w-1,2
S. worthington 2981-51	S. worthington 4586-51 1,13,23: 1,w-z	S. tel-el-kebir 13,23: d-e,n,z_{15}	13,23: z-e,n,z_{15}
S. worthington 3411-51	S. worthington 4586-51 1,13,23: 1,w-z	S. tel-el-kebir 13,23: d-e,n,z_{15}	13,23: d-1,w
S. worthington 3411-51	S. worthington 4586-51 1,13,23: 1,w-z	S. atlanta 13,23: d-e,n,z_{15}	13,23: z
S. meleagridis 3314-52	S. meleagridis 2997-52 3,10: e,h-1,w	S. bolton #302 3,10: y-e,n,z_{15}	3,10: e,h-e,n,z_{15}
S. meleagridis 3314-52	S. bolton #302 3,10: y-e,n,z_{15}	S. meleagridis 2997-52 3,10: e,h-1,w	3,10: y-1,w 3,10: e,h: e,n,z_{15}
S. meleagridis 3314-52	S. bolton	S. clerkenwell #406 3,10: 1,w-z	3,10: y-1,w
S. derby 88-54	#406 4,12: 1,w: e,n,x	S. kisangi #235 4,5,12: a-1,2	4,5,12: 1,w-1,2
S. jerusalem 258 6,7:1,w-z_{10}	S. oslo 1954-49 6,7: a-e,n,x	S. colorado 246 6,7: 1,w-1,5	6,7: a-1,5 6,7: 1,w-e,n,x
S. jerusalem 258 6,7: 1,w-z_{10}	S. colorado 246 6,7: 1,w-1,5	S. oslo 1954 6,7: a-e,n,x	6,7: a-1,5